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AUTHOR Goldstein, Laurence; Harris, Roy
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ABSTRACT

In a statement-response-reply format, a proposition concerning the study of semantics is made and debated in three papers by two authors. In the first paper, it is proposed that semantics is not the study of the concept of meaning, but rather a neurolinguistic issue, despite the fact that semantics is linked to context. It is argued that semantic research, based in cognitive science, should focus on providing: (1) a neuroscientific account of how the brain processes sensory and other input when that input calls for a verbal response; (2) a psycholinguistic account of the relation of thinking to speaking; and (3) a philosophical/linguistic account of the relation of context to the content of an utterance. In the response, this proposal is criticized as confusing sense with reference and as rejecting common sense in favor of intellectual amusement. The original author's reply answers specific criticisms and identifies inconsistencies in the respondent's work. (MSE)

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A PROGRAMME FOR SEMANTICS

Laurence Goldstein
University of Hong Kong

SEMANTICS AND ITS CRITICS

Roy Harris
University of Hong Kong

SEMANTIC SHAMANTICS

Laurence Goldstein
University of Hong Kong

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A PROGRAMME FOR SEMANTICS

Laurence Goldstein
University of Hong Kong

Introduction

A simple and, you might think, almost incontrovertible claim is that human beings frequently say things and mean what they say. This ability may not be the unique possession of humans, but it is striking, something well worth having, and something which those of an enquiring mind would want to investigate. And indeed there are people who do just that, and their investigation is known as *semantics*. But, in recent times, this apparently worthy enterprise has been savagely assaulted. It has been claimed not just that the methods of semanticists are sloppy, nor just that the theories proposed have been facile, but that the very enterprise of theorizing about meaning is misconceived.

The titles of various recently published books give some idea of the nature of the attack: *Language, Sense and Nonsense*, by Gordon Baker and Peter Hacker (1984: 389), attempts to prove that theoretical linguistics is one of the pseudo-sciences of the age, that theories of meaning "are not merely confused, but also lack any purpose". *Remnants of Meaning*, by Stephen Schiffer (1987), argues that when confusion and error have been stripped away from all current theories of meaning, the remainder is zero. *Has Semantics Rested on a Mistake?*, by Howard Wettstein (1990), answers 'Yes - a big one'. *The Language Myth*, by Roy Harris (1981), pursues an enquiry which is continued in *The Language Machine* (1987), where it is not so much claimed that semantics is impossible, but that its pretentions to being a *scientific* study must be exposed.

Scepticism about *meaning* really erupted in the early 1950's, the classical document being W.V. Quine's paper 'Two Dogmas of Empiricism' (Quine 1953). In this paper, Quine argued that no account could be given of *synonymy* without invoking one of a set of mutually interdependent and equally suspect notions such as *analyticity*, *necessity* and *definition*. Now, just as the failure to find an adequate criterion for determining whether two objects have the same *length* would cast doubt on the notion of *length*, so the impossibility of producing a workable account of *sameness of meaning* places the concept of *meaning* in jeopardy. And by 1960, in Chapter 2 of his wonderful book *Word and Object*, Quine had considerably strengthened the assault by arguing that *translation* is radically indeterminate. He undertook to show that one could produce, for a given language, several translation manuals all incompatible with each other, but each compatible with all the possible evidence one could accumulate about the verbal dispositions of speakers of that language. If we consider any sentence of that language and ask 'What is its meaning?', this sounds a pretty silly question if many different, mutually incompatible, translations are available with provably nothing to choose between them.

Wittgenstein (1958: 1-4) enjoined us to ask not for the meaning of an expression but for the use, and Chomsky, like Bloomfield before him, advocated a semantics-free linguistics. There are thus several kinds of scepticism about meaning. Following Desmond Allison, we could formulate those adumbrated above as follows:

1. The very idea of theorizing about meaning is misconceived.

2. Semantics should not be seen as scientific.

3. Semantics should be excluded from linguistics.

Although I regard 3. as the most fantastic of these claims, I shall not be challenging it here. My targets are 1. and 2.. I wish to show that semantics is a respectable pursuit and to outline a programme for its study. This is a new programme, for I share with those authors mentioned in the first paragraph most of their misgivings about the way semantics has been pursued both in the philosophy of language and in theoretical linguistics. I say that the programme is new, but Schiffer (1987) does point in the same direction, and a new book by McGinn (1989) makes what I think is a weighty contribution to the kind of programme I have in mind. Let's begin with a philosophical conundrum.

Kripke's Puzzle

About ten years ago, Saul Kripke presented philosophers with a puzzle that concerns the propositional attitude of *belief* (Kripke 1979). A great virtue of this puzzle is its simplicity. That is to say, one can get into the problem, and be led into contradiction, without straying outside common sense. Principles which intuitively seem quite commonplace and acceptable are seen to generate an absurd conclusion. This implies that, to get *out* of the puzzle one needs to abandon at least one commonsensical principle. And this is what makes it a philosophically interesting problem. It goes deep and forces us to inspect assumptions that we would otherwise carelessly accept. The solution of the puzzle, I shall argue, lies in achieving a proper understanding of the relation of *thinking* to *saying*, and thus has profound bearings on the question of how we mean what we say.

Kripke's story goes something like this. Pierre, an intelligent but monolingual Frenchman living in France, has never been to England. But, on the basis of what others have told him and from seeing pictures in magazines, he concludes that the place called 'Londres' is beautiful. He thus asserts 'Londres est jolie'. Later Pierre goes to England, learns English by the direct method (i.e., without using any translation of English into French), and fetches up in a seedy part of London. None of his new neighbours speak any French, and they, like he, rarely leave the neighbourhood. The months drag by, and, Pierre gets depressed about this town called 'London' in which he is living. He asserts 'London is not pretty'. What Pierre doesn't know is that London = Londres. Were an English-speaking friend of Pierre's to report the views that Pierre held in France, she would say that Pierre believed London to be beautiful. And even if you went up to Pierre now and asked him in French about Londres, he would enthusiastically maintain 'C'est joli', and then perhaps add ruefully 'mais London n'est pas jolie'. Yet Pierre isn't an idiot normally prone to uttering contradictions.

So, does Pierre, or does he not, believe that London is pretty? We should be inclined to say that he *does*. For, when he was living in France he had evidence as good as that of most of his countrymen for so believing and, even now, when speaking French, he speaks highly of the beauty of Londres. We should also be inclined to say that he does not believe London to be pretty. For his evidence for that belief is comparable to that of the Londoners with whom he now lives. So it is not just Pierre who is in danger of falling into contradiction; *we* who are reasoning about him find ourselves inclining towards the contradictory belief that he both does and does not believe London to be pretty.

Contradictory statements and contradictory beliefs

How should this puzzle be tackled? In another paper (Goldstein, 1990), from which the above description of Pierre's predicament is extracted, I have offered a solution. The details of this need not concern us here, but, since I shall make use of the conclusion, I need to provide an outline of the argument.

The first step is to grant that Pierre does make contradictory remarks about London. Roy Harris (private correspondence) is unwilling to grant this, but I think the following consideration is decisive: "It is impossible for the same thing at the same time to belong and not to belong to the same thing and in the same respect." (Aristotle's statement of the Principle of Contradiction at *Metaphysics* Gamma 3, 1005b18-23). "A rose by any other name would smell as sweet." (Shakespeare, *Romeo and Juliet*). Amalgamating these two undoubtedly true claims, we have: 'It is impossible for the same property at the same time to belong and not to belong to the same object, by whatever name that object is known.' That's why Pierre utters a contradiction when he says 'Londres is pretty, but London is not'. Harris says that one "cannot generate a contradiction out of nothing more than ignorance of an identity". But why he says this eludes me. I should have thought that most of our contradictions arise out of ignorance.

Given Kripke's position that the semantic function of proper names is solely to refer to their referents, it is hard to see how he could escape the conclusion that, when Pierre says 'Londres est jolie et London n'est pas jolie', he is contradicting himself in that he is making opposite claims about the same town. Scott Soames, who also accepts this point, comments that "this just shows that in certain cases one may be in no position to determine the consistency of one's statements" (Soames 1988: 213).

Soames also claims, however, that the same is true of one's beliefs, i.e., that, without making any mistake in logic or reasoning, one may be in no position to determine the consistency of one's beliefs, so, in particular, we shouldn't be too surprised at Pierre's plight. But, when you stop to think about it, this latter claim of Soames's is mistaken. We can certainly concede to Soames that all of us, except those from the planet Vulcan, have beliefs the consequences of which are inconsistent with some of our other beliefs. And those of us who are mentally disturbed have beliefs that we suppress by burying them in the subconscious. But surely if we, as it were, entertain a pair of beliefs side by side at the front of our conscious minds, then we *will be* in a position to tell whether the pair is inconsistent. Otherwise we would not know our own minds (Davidson 1987). So what I'm saying is that there is a great difference between assertions and beliefs in that we can make contradictory 'side by side' *assertions* without being aware of the inconsistency, but that the same does not hold true of *beliefs*. This conclusion clearly has consequences for the notion of *belief*. For one thing, it forces us to reject the view that beliefs or thoughts just are assertions that we haven't bothered to voice.

The structure of language and the structure of thinking

A tradition that dates back at least to Plato holds that thoughts just are unvoiced assertions. Plato described thinking as "the soul's inner dialogue with itself". Whether the medium in which this dialogue is conducted is an inner analogue of a natural language or is a universal 'language of thought' is a topic that has been much debated in the succeeding two thousand years. But, on either view, thought is thought of as having a linguistic structure. In particular, it is held that *concepts* are among the constituents of thoughts and that these are the counterparts of the *predicates* of a language. How do we tell one concept from another, i.e., how are concepts individuated? The generally accepted principle is that if it is possible for a person to simultaneously assert of some object that it is F but not G, then *F* and *G* are different concepts. But now, here comes the crunch. We have seen that it *is* possible simultaneously to assert of some object that it is pretty and not pretty. So, putting this observation together with the above principle, we obtain the conclusion that *pretty* and *pretty* are different concepts. Which, of course, they're not. Therefore we have reduced to absurdity the view that thoughts and beliefs have a structure similar to that of assertions. What we have arrived at is the thesis that the structure of thoughts and beliefs is non-linguistic.

What we are saying, then, is that a thought or belief is *represented* linguistically, e.g., by 'London is beautiful', but that the thought or belief itself is non-linguistic. This is exactly the view taken by Patricia Churchland who, in an attack on what she calls the 'sentential paradigm', cites approvingly C.A. Hooker:

> Language will surely be seen as a surface abstraction of much richer, more generalized processes in the cortex, a convenient condensation fed to the tongue and hand for social purposes. (Churchland 1986: 396).

Spoken utterances consist of reasonably short sentences because we need time to breathe. But there is no reason to suppose that the lungs are so intimately implicated in *thinking*. That stretches of thinking can be chopped up into discrete, complete units, each *the having of a thought* is an extremely dubious empirical hypothesis. Again, an utterance possesses the grammar of a particular language. In times long past, people used to believe that the grammar of a particular language (e.g., Latin or French) was the grammar of thought. There is absolutely no empirical evidence for such a belief. What evidence there is suggests that the 'grammar of thinking' is rather meagre - that which is common to the differences between all first-generation creoles and their antecedent pidgins (Bickerton 1984). Thinking is not isomorphic with saying. Someone - Pierre - may *utter* a covert contradiction. This does not show that there has to be any contradiction in the underlying *thinking*, so we need not conclude that the speaker is irrational. We can legitimately *avoid* the question of whether Pierre does or does not believe London to be beautiful by distinguishing what he would *say*, if asked, from what he *thinks*. This means that we are committed to denying that assertions are always the undistorted expressions of our thinking.

Let's look a little more closely at the process of expressing a belief or a thought. I have been urging that a belief or thought is structurally dissimilar to a verbal utterance. So expressing a thought might be compared to expressing toothpaste from a tube. The form of the expressed toothpaste (short, thin and cylindrical) is simply a result of the manner of its production (extrusion, under intermittent pressure, through a round nozzle). Neither the geometry of toothpaste (!) nor the structure of thought can be inferred from what comes out of, respectively, our nozzles or our mouths. Yet, when we wish to <u>say</u> what we are thinking, there has to be a priming stage when our thinking is linguistically encoded so that it may be delivered to the outside world. The *coded* thinking shares the structure of the linguistic utterance, but I am suggesting that distortion may occur in the coding process just as it may occur in employing a code (which might include the conventions of perspective drawing) for producing two-dimensional pictorial representations of the three-dimensional world. (Especially if you are Maurits Escher.)

Thinking and thoughts

Confusion sets in immediately we equate thinking with the having of thoughts where these thoughts are conceived of as entities having a linguistic structure. The trouble is that there is a perfectly respectable notion of a thought, viz., a speech act from which the sound has been abstracted (just as, in (Austin 1975: 92-6), a so-called 'phonetic act' is a speech act from which everything *except* the sound has been abstracted), and this clearly is a linguistic creature. It is a serious mistake to assimilate thoughts in this latter sense with elements or constituents of thinking. When I ask for your thoughts on a subject, I am not expecting a neurophysiological report. However, when I'm assessing your rationality, it is your thinking I am interested in, and I may need to discount some of the linguistic clothing in which your thinking is dressed; we do this all the time with speech-impaired people.

Although Pierre makes what *we* recognize to be contradictory assertions, these are not expressions or representatives of an underlying contradiction in his thinking. His assertions that London (Londres) is beautiful were based upon what he heard from other monolingual French

people and upon his seeing some pictures, captioned 'Londres', of nice parts of the town. His assertions that London is not beautiful were based upon his perceiving different parts of the same town. There is nothing at all contradictory in these perceptions and it is only when they are 'fed to the tongue' that contradictory assertions emerge. Let us not forget that a dog, after it has perceived a neighbourhood for a short time, can soon find its way around, and acquires many beliefs about the environment. It can do the same when placed in a different neighbourhood. The dog would not be accused of irrationality or inconsistency if it failed to appreciate that these were two quite distinct neighbourhoods *in the same town*. And Pierre is in a position similar to the dog's, at least until he starts putting names to what he sees.

The public language English contains an expression to which convention has assigned the role of referring to the city of London. If we can successfully deny that there is an element of thinking that plays a counterpart role, then we can repel, as resting on a false assumption, questions about what Pierre thinks about the city. Remember, we are not asking about what Pierre *would say* about London: the question about Pierre should be compared to the question 'What does the dog think about London?', to which the answer is 'Nothing' - unless, perhaps, the dog is viewing the whole city from the basket of a hot air balloon. In the case of humans, there is undoubtedly a process by means of which our thinkings and perceivings get translated into words, as when we report what we think or perceive. But it is a mistake simply to *assume* that, conversely, to each of our words there is a mental counterpart, for example some distinctive cluster of neural spikes.

If we have made a reasonable case that predicates in natural languages have no counterparts in thinking, then it is highly plausible to suggest that singular terms don't either. The classical grammatical dichotomy of singular term/predicate rests upon the idea that there are two heterogeneous but complementary *logical* roles for words to perform: that of *identifying* individuals, and that of *characterizing* the individuals so identified. Kant, as is well known, saw this dichotomy manifested in the mental realm in the shape of a categorial distinction between *intuitions* and *concepts*, no judgment being possible without the presence of *both* (Kant 1781: A51/B75 (Kemp Smith (trans.) 1929)). By raising doubts about one element of this complementary pair we are bringing the whole doctrine into question and *ipso facto* casting doubt on the other element. (It is fascinating, in this regard, to compare the writings of the ancient Chinese philosophers of language who, for various reasons, were not drawn towards the dichotomy that, in western theorizing, now seems so natural: see Hansen (1989).)

Two mistakes

We have distinguished *asserting* from *thinking*, where the latter is taken to be a brain process. When we say, 'After a lot of thinking, he reached the conclusion that p', we are referring to this process. But we have seen that there is another sense of 'think' much more closely related to 'assert'. Thus, to say 'He thinks that p' means, roughly that he would be prepared to assert that p. It seems to me that these two senses of 'think' have often been conflated, with disastrous consequences. I cite as an example, Wittgenstein, who writes:

> One of the most dangerous of ideas for a philosopher is, oddly enough, that we think with our heads or in our heads. The idea of thinking as a process in the head, in a completely enclosed space, gives him something occult. Is thinking a specific organic process of the mind, so to speak - as it were chewing and digesting in the mind? Can we replace it by an inorganic process that fulfills the same end, as it were use a prosthetic apparatus for thinking? How should we have to imagine a prosthetic organ of thought? No supposition seems to me more natural than that there is no process in the brain correlated with associating

> or with thinking; so that it would be impossible to read off thought-processes from brain-processes.....' (Wittgenstein 1967: ##605-608).

We may agree with Wittgenstein that we cannot read *thoughts* off from brain processes - say, because thoughts, like the contents of assertions, are in part *constituted* by context (see McGinn (1989), Recanati(1989), McDonough(1989)) - but still maintain that *thinking* is a brain process. It is only because he is guilty of the above mentioned conflation that Wittgenstein can make the mad claim that there is no process in the brain correlated with thinking.

The *opposite* mistake to Wittgenstein's, but one stemming from the same source, is made by many modern semanticists, particularly those who espouse the computational model of the brain. These writers accept that thinking is a brain process, and believe that it can in principle be described in computational terms. And, since they take *saying something with meaning* to be a form of thinking, they find it incumbent on themselves to give a computational account of semantics. Perhaps the best known exemplar is the linguist Jerry Fodor (see, e.g., his (1975, 1987)). Fodor is impressed by the fact that our belief and attitudes are *productive*. He explains this notion as follows:

> It has probably never occurred to you before that no grass grows on kangaroos. But, once your attention is drawn to the point, it's an idea that you are quite capable of entertaining, one which, in fact, you are probably inclined to endorse. A theory of the attitudes ought to account for this productivity; it ought to make clear what it is about beliefs and desires in virtue of which they constitute open-ended families. (Fodor 1985: 89).

Fodor believes that the productivity problem is solved by the idea that our thoughts have constituents that can be assembled in a variety of ways to produce completely new thoughts. These constituent symbols, he claims, are nothing other than mental representations - words in a 'language of thought' - upon which we perform computations (Fodor 1975).

Fodor's conclusion, then, is that RTM (the representational theory of mind) is a consequence of the requirement for unasserted mental sentences to be the constituents of complex propositional attitudes. His is a brain-writing thesis writ large: the words of a mental language are physically instantiated in the brain, and, just as groups of words of natural languages represent how things are in the world (e.g., when a speaker *says* how things are) so groups of words of the mental language constitute inner representational states. But the critical question to ask of Fodor is 'How do these representations represent?' i.e. how do these signs in the brain acquire their intentionality and other semantic properties? (Heil 1981). Zenon Pylyshyn answers this question by proposing that the semantical properties of representational states arise from the causal relationships which, *qua* physical entities, those states enjoy. He writes

> There is, if you like, a parallel between the behavioral patterns caused by the physical instantiation of the representational states and the patterns captured by referring to the semantic content of these states. How can this be? How can such a parallel be maintained coherently over time? Only one nonquestion-begging answer to this dilemma has ever been proposed: that what the brain is doing is exactly what computers do when they compute numerical functions; namely, their behavior is caused by the physically instantiated properties of classes of substates that correspond to symbolic codes. These codes reflect all the semantic distinctions necessary to make the behavior correspond to the regularities that are stateable in semantic terms. In other words, the codes or symbols are equivalence classes of physical properties which, on one hand, cause the behavior to unfold as it does, and on the other, are the bearers of semantic interpretations that provide the needed higher-level principles for their

> individuation and for stating the generalizations. As dyed-in-the-wool realists, we propose as the next step exactly what solid-state physicists do when they find that postulating certain unobservables provides a coherent account of a set of phenomena: we conclude that the codes are 'psychologically real', that the brain is the kind of system that processes such codes and that the codes do in fact have a semantic content. (Pylyshyn 1984: 39-40).

So, for example, the behaviour of removing my trousers may be the causal result of a number of inner states, states which under a *semantical* description are propositional attitudes such as the *belief* that I am currently wearing trousers, the *desire* to be divested of them, and, perhaps, some higher-order attitudes such as the belief that Brigitte desires to have me trouserless. Fodor has a closely similar response (consult his (1985: 91)). He argues that a computer operates on symbols which, under one description, have meaning, under another description have causal properties.

The easiest way to refute the Fodor\Pylyshyn claim is to find examples of ambiguous sentences, i.e. sentences tokens of which, while *physically* alike, mean different things in different contexts, where the difference cannot be attributed to lexical or syntactical ambiguity (so that the counterpart mental sentences would be identical). Consider first the aristocratic woman who boasts:

> 'I can get as much sexual satisfaction as I want just by snapping my fingers.'

This can be disambiguated only if one knows the cultural significance of snapping one's fingers (curtly summoning an inferior - in this case, a stud). Consider also

> 'The headmaster said that a pupil would be killed unless something is done about road safety provisions near the school.'

The ambiguity of this can be discerned by comparing it with 'The terrorist said that a passenger would be killed unless....', i.e. the headmaster's statement could be read as a *threat*, but it isn't - it would normally be read as a *prediction*. So, to understand these two statements, we need to know about the institutions of threatening and predicting, and we need to know something about the different roles played by terrorists and headmasters. It seems clear that we are now well outside the territorial waters of (deep) syntax and the causal roles that sentences *qua* physical entities can play.

Thus we have to reject a computationalist account of semantics, the fundamental reason being that computationalism is an *internalist* doctrine since the relevant computations are upon states of the brain. Yet two speakers can have identical internal states, and be making the same utterance, yet what they *mean* be entirely different - the classical argument is to be found in (Putnam 1975) and is developed in (McGinn 1989).

We can reinforce this point by reverting to what is correct about Wittgenstein's position. For Wittgenstein, the *semantics* of an utterance is its use in a context (Wittgenstein 1953: #525) where a context is "not the mere physical environment of the utterance, but is the institutional and cultural background of the utterance. A context, so conceived, is 'significant' in the sense that it is constituted by *rules, procedures, norms and the like*" (McDonough 1989: 7, cf. Wittgenstein 1953: #337). Normative constraints for the use of words "arise because that use is shaped or gradually mastered within an experience of normative or conventional practices which enable communication and mutual understanding" (Gillett 1988). The use of a word, says McDonough, is embedded in its situation:

> "The key notion in Wittgenstein's notion of use is that of *context embeddedness*...This embeddedness is not constituted by causal connections

between utterances and contexts, but by criterial or conventional connections between them. The description of the use of an utterance is, therefore, nothing like the description of a physical state. It is more like the description of a *criterial connection* between words and significant contexts." (McDonough 1989).

These sketches of Wittgenstein's position are not transparently clear, but I am not yet able to offer any significant improvement. Nor have I done anything to figure out the relationships between the Wittgensteinian and the Putnam\McGinn accounts of how context enters into the content of an utterance. All this work remains on the agenda.

Conclusion

Semantics is not the study of the concept of *meaning*. This concept may well be obscure or incoherent, as many of the authors we mentioned at the beginning aver. But that does not imply that the *phenomenon* of *meaning what we say* is chimerical. I have argued that, although *meaning what we say* is not a *biological* phenomenon - one that can be explained entirely in neurobiological terms - the role of the brain cannot profitably be ignored. But the connection between *thinking* and *saying* is by no means simple; in particular, I argued, the two are not isomorphic. And this is primarily because the content of one's sayings is essentially context-involving. What is required, then, is a neuroscientific account of how the brain processes sensory and other input when such input calls forth a verbal response, a psycholinguistic account of the relation of *thinking* to *speaking*, and a philosophical/linguistic account of the relation of context to the content of an utterance. In short, this is a project for cognitive science.

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SEMANTICS AND ITS CRITICS

Roy Harris
University of Hong Kong

East and West, philosophers have an inordinate fondness for espousing theses which are baffling to common sense. (Parmenides' claim that nothing changes and Kung-sun Lung's famous denial that a white horse is a horse are examples that spring to mind.) They also have a penchant for using key words in ways opaque to non-philosophers. When both these traits are combined, it does not make for clarity of communication. This is the difficulty I have with Dr. Goldstein's paper 'A Programme for Semantics', and in particular with his use of the term *semantics*.

Now I would be the first to admit that the term *semantics* does get used in some astonishing ways nowadays. Very recently I was reading a paper which purported to inform me (quite seriously, as far as I can judge) that scientists now know that the operations 'critical to semantics' take place in 'cortical areas underneath your forehead'. Furthermore, it appears, I might even be able to observe them taking place (and in technicolour) thanks to the newly developed technology of positron emission tomography.

'Watch yourself doing semantics: it's on the screen *now*!'

But is it? I doubt it. And I doubt that it ever will be, however refined the techniques of PET become. The patches of colour on the screen tell me no more about semantics than a comprehensive brain-scan of Boris Becker could tell me about tennis.

Perhaps I should add that this article on PET began with a quotation from T.S. Eliot, in which J. Alfred Prufrock complains that it is impossible *to say just what he means*, and fantasizes that it might somehow be possible to make visible his desperate cerebral search for the right words, 'as if a magic lantern threw the nerves in patterns on a screen'. Precisely thus, the article claimed, PET technology could now reveal 'the mind in motion'. It blew the trumpet of 'cognitive science' loud and long. I probably would not have remembered reading it but for the arrival on my desk of Dr Goldstein's paper, which is far more serious stuff.

I find myself listed in the opening paragraph among those who in recent years have promoted or taken part in a savage assault on the 'worthy enterprise' of semantics. To this savagery, it appears, I have devoted at least two books, or parts thereof, over a period spanning most of the past decade. Clearly I am a bad guy, because only bad guys savagely assault worthy enterprises. And those who return to their savagery unsatiated over a period of years must presumably count among the most sadistic butchers.

Given the charge, it would be a feeble defence simply to say that I think Goldstein has got it all wrong. So I will try to muster some evidence which at least suggests that he may have done so. Although he mentions two books of mine, he does not refer to the first book in that trilogy, *The Language-Makers*, which was published in 1980, one year earlier than *The Language Myth*. Now a reviewer in the *Canadian Philosophical Review* told the readers of that learned journal that *The Language-Makers* was 'a defense of structural semantics'. So unless anyone contends that I had completely changed my position between 1980 and 1981, it seems that there is something odd here. No one who has reviewed all three volumes of the trilogy has (so far) accused me of shifting

my ground in the course of it. I would be surprised if such an accusation would even occur to any intelligent reader, and even more surprised, I need hardly add, if the accusation could be substantiated.

Let me make it quite clear that I am not here endorsing the particular interpretation of *The Language-Makers* given in the *Canadian Philosophical Review*; but at least the writer in that journal showed an awareness of something which Goldstein's article ignores entirely; namely, that there are a number of quite distinct forms of academic inquiry which have, at various times and places, been called *semantics*.

The English term *semantics* is a translation of the French *sémantique*, coined in the late nineteenth century by Michel Bréal. It was originally applied to the study of changes of meaning (e.g. of the kind documented in the columns of the *Oxford English Dictionary*) and is still used in this sense. Somewhat later it came to be used to designate a branch of synchronic linguistics within the structuralist framework inaugurated by Saussure. Third, it was adopted as the designation of one of the three subdivisions of semiotic, as developed by Rudolph Carnap and his followers. Fourth, it was taken over as a general rubric in academic philosophy under which to place a certain range of traditional problems related to the notion of truth (but which had never before been called problems of 'semantics'). Fifth, it was appropriated by experimental psychologists, under whose aegis it became a cover term for the investigation of word associations and language-related aspects of memory. But in none of these various and quite separate enterprises was semantics ever what Goldstein's (presumably naive) reader is invited to believe, i.e. the investigation of the ability of people (other than J. Alfred Prufrock) to 'say things and mean what they say'. *A fortiori*, his description of my work and that of others listed in his opening paragraph as attacks upon this 'worthy' (but non-existent) endeavour is a misdescription.

Nor did scepticism about meaning 'really erupt' with Quine in the early 1950s, as Goldstein says it did. It had already been erupting periodically for a least a generation before that. The Mount Vesuvius of them all occurred when Quine was still a student. This was the behaviourist cataclysm of the 1920s, which had a more devastating effect both inside and outside the walls of philosophy than all the publications of Quine put together.

Scepticism about meaning was in any case a much more pervasive cultural malaise, by no means confined to any of the academic branches of semantics mentioned above. The 'literary' manifestation of this scepticism is represented by such figures as Andre Breton and James Joyce. (Did anyone mention T.S. Eliot?) Its popular brand was marketed by Orwell. And scepticism about translation (Quine's second great contribution, according to Goldstein's potted history of semantics) has a longer history still, dating back to Classical antiquity.

Even if Goldstein's historical framework were not askew from the start, the gems of misinformation framed therein would glitter in their own right. The reader is told that both Bloomfield and Chomsky 'advocated a semantics-free linguistics' (an assertion that immediately leads one to wonder whether its author can ever have read carefully the works of either). The theoretical positions taken by Bloomfield and Chomsky on this are quite different. Chomsky began his career arguing for the *independence* of grammar from linguistic meaning (which is by no means the same as advocating 'a semantics-free linguistics'), but his later work is very much concerned with issues in (one branch of) semantics. As for Bloomfield, on the other hand, his linguistics, far from being 'semantics free', was actually semantically based. Bloomfield's mature work in linguistics included devising a whole new terminology for semantics. No wonder Goldstein, having ignored all this, feels free to proclaim himself the champion of a 'new programme' in semantics.

Any programme, however, deserves to be judged on its own merits, and not just by the rhetorical claims of its promoter. Inspection of the programme, unfortunately, merely deepens my perplexity about what Goldstein's 'semantics' is supposed to be. The new programme is ushered in with a second-hand conundrum borrowed from Kripke. Even first-hand, the conundrum in question was never very puzzling. We are spun an unconvincing philosopher's yarn about a dimwitted Frenchman, Pierre, who hasn't worked out that the same city is known as *London* in English but as *Londres* in French. Zo ee mekk ze beeg goof of assigning incompatible predications to these two cities 'London' and 'Londres'. One he finds pretty and the other not (and this on the basis of what he himself has seen). Poor Pierre. Nil out of ten, if not *zéro*. And from Pierre's low score in the predication test, we are invited to conjure up a beeg problem for semantics to solve.

The sheer farce of this example is that there is no problem at all. Pierre is not guilty of any contradiction, dim though he be. Goldstein cites me as claiming that one 'cannot generate a contradiction out of nothing more than ignorance of an identity' and quotes an amalgam of Aristotle and Shakespeare against me. Obviously, this must be another instance of my predilection for savaging semantics.

Now satisfying though it would be to outshine the combined forces of The Philosopher and the Swan of Avon in one glorious joust, I fear that neither of these two worthies would be prepared to enter Goldstein's lists against me on this issue. For neither independently makes any claim which would lay a contradiction at poor Pierre's door. Goldstein tries to make out that both Aristotle and Shakespeare subscribe jointly to some such declaration as: 'It is impossible for the same property at the same time to belong and not to belong to the same object, by whatever name that object is known.' That is stretching the evidence, to say the least. As far as I know, Aristotle never discusses substitution of synonyms as a logical problem, while Shakespeare never discusses the Principle of Contradiction. So the team against me is one that only manager Goldstein has fictitiously cobbled together.

The plain common sense of the matter is that it quite obviously *does* make a difference whether Pierre knows that *London* and *Londres* designate the same city or not. And he might indeed be guilty of a contradiction if he said, for instance, 'Londres est jolie, mais London n'est pas jolie; et c'est curieux, parce que c'est la même ville'. (Or he might just be joking.)

The odd thing to me about Goldstein's position (unless he is joking too) is that one of today's philosophical idols - Frege, no less, who was, according to Dummett, 'the founder of modern semantic theory' - pointed out a long time ago that if A and B both use the name *London* but each attaches different items of knowledge to this name then they are - from a logical point of view - speaking different languages. (So if both utter 'the same' sentence of the form 'London is *p*' they do not necessarily agree with each other, while if A utters it and B does not concur they are not necessarily contradicting each other.) I would have thought that the moral of this lesson in the case of Pierre hardly needs spelling out. Yet, bizarrely, Goldstein wants to accuse Pierre of falling into contradiction *only* when his perceptions of different parts of what is in fact (unknown to him) the same city are 'fed to the tongue', i.e. put into words. On the contrary, *pace* Goldstein, that is just what Pierre in his innocence avoids, because when he verbalizes his impressions he uses the word *Londres* to designate the city he finds pretty and the word *London* to designate the city he does not find pretty.

Pierre, bless him, *does* mean what he says. And that, ironically, is just the bit Goldstein's 'semantics' cannot cope with.

It is not my purpose to criticize the rest of Goldstein's paper in detail, but here are two general comments. (1) Much of Goldstein's case seems to rest on an elementary semantic confusion of sense with reference (a suspicion reinforced by his invoking briefly in support an argument of Putnam's which is notorious for this very confusion). (2) The paper seems to be infected throughout with a misguided zeal to reject common sense and bemuse us with alleged riddles in 'semantics'. For instance, Goldstein at one point tries to show that it is impossible to have two different concepts of prettiness (or of anything else, for that matter). And this in turn supposedly goes to show that, in spite of naive impressions to the contrary, the lay belief that we think with words is totally mistaken. A great triumph for the new programme! When 'semantics' gets to that stage of Byzantine obscurity, perhaps it is best patted on the head and left to bemuse itself.

The only positive contribution I can offer to Goldstein's new programme is to try to pinpoint exactly where it goes off the rails. The point is at that tricky junction where a switch is made from talking about language to talking about 'thinking'. Goldstein appears to regard thought as a kind of mental toothpaste in a tube, which is just goo until squeezed out linearly into words. I do not wish to get involved here in yet another argument about the linearity of the linguistic sign (having already made in print my fair contribution to that perennial debate). But if you want to claim that thinking is something quite independent of and prior to verbalization, as Goldstein's 'toothpaste' analogy seems to imply, then you cannot at the same time claim it has anything to do with semantics, which - at least, on all accounts I know of- is by definition concerned with language. So 'toothpaste' semantics is not merely an amorphous but an incoherent enterprise. *There is no toothpaste in the tube.*

Apart from the metaphor, perhaps, this is hardly news. The point has been argued by many theorists who have written about language, including Saussure and Wittgenstein, and Goldstein gives us no reason (apart from all the nonsense about Pierre) for concluding that they were wrong. But if all Goldstein means by his toothpaste metaphor is that different languages are semantically anisomorphic in various respects, that is hardly news either. (In the semantics business, it was announced decades back in a more interestingly controversial form usually associated with the names of Sapir and Whorf.)

When I reason that if all men are mortal and Socrates is a man, then Socrates is mortal, it is quite implausible to suppose that I first of all go through some entirely non-verbal 'thinking' process, and then search around for a linear sequence of appropriate words in which my 'thinking' may. if need arise, be announced to the outside world. Or if I do, then I can tell you nothing about it, and nor can anyone else. Nor would PET pictures of my cerebral processes during the timespan in question throw any light on the matter. There is no reason for supposing that I engage in any such mysterious process at all. For the notion that my mind can somehow 'entertain' these particular propositions in a totally abstract form devoid of all verbal trappings just does not make much sense. Any question about the 'semantics' of my reasoning only arises once there are words to consider. And the words I use are not patches of colour on a PET screen.

This is not to say that I am in any way committed to the view that *all* kinds of thinking are verbal. Max Müller's famous dictum 'No thoughts without words' (which presumably for Goldstein epitomizes most of the fallacies he sees himself as attacking) certainly does not apply to all forms of communication (and I am not sure that Müller intended it to). For example, it seems to me that a painter often thinks with lines, shapes and colours, and not with words. But in the case of the painter, it also seems to me quite gratuitous to suppose that before the painting is started there occurs an abstract 'thinking' process in the painter's head in which lines, shapes and colours do not feature at all.

Finally, where language is concerned, I am aware that in certain areas of so-called 'cognitive science' the assumption is nowadays made that we would not be able to speak at all unless we had previously 'internalized', at a level far below that of consciousness (but doubtless in principle accessible to PET pundits in the Mallinckrodt Institute of Radiology) the unimaginably complex rules of a language. Whether a similar claim is made about painting I do not know. As far as the linguistic claim is concerned, I have discussed it elsewhere ('The Grammar in Your Head', in *Mindwaves*, ed. C. Blakemore & S. Greenfield, Oxford, 1987) and do not wish to regurgitate that discussion here. The only point I would like to add is that 'cognitive scientists' who can swallow that kind of story about *speaking* doubtless can also swallow a parallel story about 'thinking' to go along with it. If they do, then I have a simple terminological recommendation to make: that 'cognitive science' should now be rebaptized 'cognitive mythology'.

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SEMANTIC SHAMANTICS

Laurence Goldstein
University of Hong Kong

Many readers of this journal will, by now, have become accustomed to Professor Harris' polemical outbursts, in seminars, on the radio and in print. By these standards, his 'Semantics and its Critics' seems quite bland. But this innocuous appearance is deceptive. Committing numerous errors and calling up unscrupulous rhetorical ploys as he proceeds, Harris has set out to beguile his readers and to befog the issues. My reply is an attempt to clear the air.

Harris' paper combines erudition, philosophical sophistication and wit in equal (if small) measure. The main ingredient, as I have suggested, is fog. This being so, all I can really do is to wander around in the murky prose and, when I can identify them, respond to his various points. (I use the word 'point' in the Euclidean sense - something having no breadth or depth, where two or more lines get crossed.)

The Fear of Science

I suspect that, like Wittgenstein, Harris sees science as wreaking destruction on the cultural landscape as it remorselessly advances into every area of enquiry. He has a visceral distaste for science, a fear of the unknown. Like his ex-Oxford colleagues, Peter Hacker and Gordon Baker, he wishes to insist that science certainly has no place in semantics. In their vastly entertaining book, *Language, Sense and Nonsense*, Hacker and Baker argue that the theories of meaning proposed by modern linguists and philosophers in the spirit of a science of language are responses to bogus problems. Now, I have a great deal of sympathy with this view, but here is what I take to be a non-bogus problem: Human beings have semantic ability - they can say things and mean what they say. A parrot, although it can utter words, does not have this ability. What exactly is it that we have, but parrots lack?

Is there nothing to be explained here? If there is something to be explained, is it at all likely that neuroscientific findings about the differences between human and parrot brains will be *completely irrelevant* to the explanation? If not, then science does have something to do with semantics in so far as it has some contribution to make to the understanding of semantic competence. There are a number of things that Harris could say at this stage. Far be it from me to guess what *precisely* he would say, but he would undoubtedly charge that I have been using the word 'semantics' in a non-standard way. More on that in a short while.

In W. Calvin and W.J. Ojemann's (1980), you can read about a man who had a minor stroke that left him with a remarkable speech defect. All his utterances were as grammatically correct as those of a normal person, except that, whenever he should have used a noun, he always inserted the phrase 'affirmative action'. There is now a great deal of evidence about the relation of different brain lesions to different kinds of speech defect. For example, it is a fact that lesion in the angular gyrus typically leads to impairment of naming ability. Discoveries such as these excite my interest. It seems obvious that such neurophysiological research will throw much light on human linguistic skills. Professor Harris is merely scornful about the possible relevance of such work. His remarks on positron emission tomography give you the flavour.

Facile Solutions

Kripke's puzzle about Pierre has been troubling philosophers and others for over ten years; a collection of essays on it and related problems has recently been published in the *Oxford Studies* series. Naturally enough, Harris thinks that the conundrum is just an 'unconvincing philosopher's yarn', and that he can solve it at the stroke of a pen. Unfortunately his solution (which, as he acknowledges, is taken from Frege) just won't work. One cannot expect a busy man like Professor Harris to keep up with the literature outside his own subject, so I don't really want to criticize him for being unaware of the powerful body of arguments assembled over the last twenty years which have shown that the 'description theory' of proper names, on which he relies, is untenable. (I could give him a reading list, at the top of which would be the introduction to the second edition of Kripke's *Naming and Necessity*.) Nor would I expect him to know that his proposed solution is considered and rejected in the original article in which the puzzle about Pierre is propounded.

What I can criticize Harris for is a failure to observe the ludicrous consequences that his application of the discredited Fregean theory lands him in. For Harris is led to the conclusion that Pierre "uses the word 'Londres' to designate the city he finds pretty and the word 'London' to designate the city he does not find pretty". Well, are these cities the same? If they *are*, then my affirmation that Pierre falls into contradiction resists Harris' denial. If they are *not* then where *are* these two cities that Harris' sorry tale leads him to postulate? They are certainly not co-inhabitants of the earth's surface, like London, England and London, Ontario. So perhaps they just exist in Pierre's mind. So that when Pierre says to his French friends 'Je vais à Londres' they don't wish him a pleasant trip, since he's only going on a flight into his own imagination.

Kripke has some good advice in his original article: when people like Harris purport to solve the puzzle by describing Pierre's situation in various ways, press upon them the question 'Does Pierre, or does he not believe London to be pretty?'. That's a good, straight question that *I* should like to pose to Harris. I'm not asking whether Pierre finds pretty the parts of London that *I* know or the picture of London that *I* have in my imagination. I don't think that Harris' solution makes available to him a straight answer to this question.

Harris magnanimously offers to identify the exact point at which my programme 'goes off the rails'. It is, apparently, where a switch is made from talking about language to talking about thinking. In my paper, I compared the expression in words of what someone is thinking to the squeezing of toothpaste from a tube. The point of the analogy, of course, was to illustrate my claim that the shape, or structure of a spoken or written language is quite unlike the structure of the underlying thinking. Now the cheapest thing that you can do to an opponent's analogy is to press it in a direction that you know full well your opponent did not intend it to be pressed. And sure enough, Harris adopts this ploy, and tries to foist upon me the claim that thought is just completely unstructured goo and is quite unrelated to language. I, of course, make no such claim.

I do, however, hold that thinking is non-verbal (which is not to deny that the development of one's thinking is influenced by the development of one's language). Professor Harris takes a contrary view; he holds that some kinds of thinking, for example, *reasoning* are verbal. Judging by remarks he makes towards the end of his paper, I gather that he thinks that, with the 'goo' theory sidelined, an account of (many sorts of) thinking as verbal is the only game in town. Well, he may have thought that when he was writing his paper, but since then he has attended a literature review seminar of mine on Colin McGinn's *Mental Content*, and will now know that the theory of *mental models* (where the models are simulations, not representations) is a serious contender. The locus classicus for that theory is Kenneth Craik's (1943), and a better known recent work is Philip Johnson-Laird's (1983). I do not have the space here to mount a full-scale exposition and defence of the theory of mental models, but I can just briefly mention three respects in which it scores over the sentential view. First, it allows room for crediting language-less creatures with thinking;

second, it squares better with experimental results obtained by psychologists on reasoning-speed (see Johnson-Laird, *op. cit.)*; third, it makes more sense biologically (see McGinn, *op. cit.*, Patricia Churchland, *Neurophilosophy*, Ruth Millikan (1984)). All of the works mentioned in this paragraph are tough going, and, of course, it's much easier to ridicule them than to read them.

Sophistry

Harris seeks to answer my charge that he is among the writers who have lately mounted an assault on semantics (I said that he 'not so much claimed that semantics is impossible, but that its pretensions to being a *scientific* study must be exposed'). You might think that, with so many best-sellers under his belt, it would be extremely easy for him to refute me simply by quoting one or two short passages. But no. Take a look at the convoluted procedure he adopts: First he quotes a reviewer of *The Language-Makers* (the first book in a Harris trinity) as saying that that book was 'a defense of structural semantics'. Then he indicates (he wouldn't, of course, just say it) that in the subsequent two books he did not shift his ground. We are being encouraged, it seems, to infer that Professor Harris is a steadfast defender of structural semantics. Wrong! For the next thing that Harris says is 'Let me make it quite clear that I am not here endorsing the [reviewer's] particular interpretation of *The Language-Makers*'. So what is the upshot? Does Harris favour structural semantics or doesn't he? Has he denied my characterization of his attitude to semantics, or hasn't he? We just can't tell. There seems to be nothing solid to grasp behind the veil of bluster and sophistry.

While we're on the subject of sophistry, let me expose another couple of examples from Harris' paper. At one point in my paper, I amalgamated a thesis of Aristotle's with one of Shakespeare's to produce a proposition with which one of Harris' claims was inconsistent. If the amalgam proposition is true, then Harris' is false, simple as that. It is, of course, open to Harris to show either that the Aristotelian or the Shakespearean thesis is false, or that truth was somehow a victim of the amalgamation process. But all of these three moves are intellectually respectable, so one would not expect Harris to avail himself of them. What he does instead is to insist, totally irrelevantly, that neither Aristotle nor Shakespeare *separately* maintained the amalgam proposition. Similarly: '2 + 3 = 5. Therefore 5 is not greater than 4, because neither 2 nor 3 is *separately* greater than 4.' Great argument. 'Surely Professor Harris' mistake can't be as crass as that', you say. Take another look at his paper, and let me know.

A second example. In the process of constructing a *reductio ad absurdum* argument, I tried to show that Pierre, when looking at pictures of nice parts of a town, might say that the town was pretty (jolie) but, when stuck in a shabby part of town, and not knowing it to be the same town as the one in the pictures, might say that the town was not pretty. He would be using the word 'pretty' in the same sense on both occasions. Somehow Harris conjures out of these remarks of mine the view that 'it is impossible to have two different concepts of prettiness (or of anything else for that matter)'. Huh? - or, as a chess columnist would put it, '??'. I should actually put it more strongly than '??', which means only 'highly questionable' - there is no question but that I never said anything remotely like what Harris says I said.

In the same paragraph, he says that I confuse *sense* and *reference* and hints that I've fallen under the bad influence of Putnam who is 'notorious' for being guilty of this confusion. Now, the distinction between sense and reference, which was made by Frege in 1892, is familiar to just about every philosopher. The word 'water', for example, refers to water; the *sense* of the word contains what Frege calls a 'mode of presentation' and can be expressed in a descriptive phrase which nowadays would include the term 'H2O'. This example, I hope, clearly illustrates the distinction. It is, by the way, the very example that Hilary Putnam uses in the very argument to which Harris alludes.

Scepticism about Meaning

Many forms of academic enquiry go under the title 'semantics', and Professor Harris supplies a somewhat incomplete list of these. In general terms all these enquiries are concerned, in one way or another, with *meaning*. Because, like Harris, I prefer homely, commonsensical locutions to dry, scholarly ones, I opted to characterize semantics as having to do with people meaning what they say, thus avoiding the rather pompous expression 'an investigation into the nature of meaning'. The paraphrase, though by no means perfect, was, I thought, innocuous enough, and had the virtue of picking out a *phenomenon* with which we are all familiar, rather than a *concept* that has proved worrisome to some. I don't know why Harris objects to my paraphrase, or thinks that I have twisted the meaning of 'semantics'. Perhaps he thinks that 'to mean what one says' has more to do with *threatening* than with *meaning*. If any readers share Harris' worries, let me reassure them that, like anyone else who writes on semantics, I am talking about *meaning*.

As to when scepticism about meaning really erupted, this question may not be one that keeps readers of this journal awake at night, so I'll be brief. In the early 1950s, a group of philosophers including W.V. Quine, Benson Mates and Nelson Goodman were arguing for the quite specific thesis that the concept of *meaning* is incoherent. If that thesis is correct, the consequences are far-reaching. The belief that there is a sharp division between analytic statements (those true or false in virtue of meaning alone) and synthetic ones would be exposed as mere dogma, and the idea that certain statements (including the laws of logic) do not have to face the tribunal of experience, and are immune to revision, would have to be jettisoned. At least, so Quine argued.

Harris urges that a more devastating sceptical eruption occurred thirty years earlier with the advent of Behaviourism. Although I am ready to acknowledge that the history of ideas is not my forte, it seems to me that a couple of points tell strongly against Harris' suggestion. First, we find behavioural (or 'mechanistic') linguists such as Bloomfield offering behavioural *accounts* of meaning. That hardly betrays a scepticism about the concept of meaning - quite the opposite - it just evinces their belief in the inadequacy of competing analyses. Second, behaviourism, in its stark form, denies the existence of all mental states and processes. So meaning would be condemned only if it were deemed a mental state or process, or were implicated in such, and it would fall under a blanket criticism rather than being subject to a specific attack. A similar point can be made about translation. Quine's thesis of the indeterminacy of radical translation really was epoch-making because the argument convinced many people that there is no such thing as the translation of one language or idiolect into another. That's very different from harbouring a suspicion that sometimes interpretation is impossible or is necessarily inexact - a scepticism that has been around ever since people tried figuring out what others were saying to them.

One last little nit to pick: My understanding of Chomsky's early position is that he held that, whereas the rules of syntax could be brought under the control of formal, mathematical techniques, meaning is by nature unformalizable and is therefore best excluded from a science of language. I therefore described him as advocating a 'semantics-free linguistics'. I don't think that this is misleading, but I have no particular objection to Harris' wetter formulation either.

A Premature Apology Retracted

In the last paragraph of an earlier draft of this paper, I had written: 'Finally, I should like to mention one matter over which I unreservedly acknowledge Professor Harris to be right and myself to be wrong.' I was referring to the description of Bloomfield's work that I had given in 'A Programme for Semantics'. In his reply, Harris had said 'As for Bloomfield ... his linguistics, far

from being semantics-free, was actually semantically based.' I checked Bloomfield's *Language* (1933), and indeed found my characterization to be inaccurate.

How could I have come by this misconception? A friend has now identified one likely source. The following account of Bloomfield's position appears in a work that I had rather casually perused a few months earlier:

> '...distributional analysis dispenses with meaning altogether, treating linguistic structure as purely syntactic in nature. This elimination of meaning (a policy in line with then fashionable behaviourist theories in psychology, and officially espoused by Bloomfield in the 1930's as the only 'scientific' basis for linguistics) left linguists with nothing synchronic to describe except sequential patterns of substitution and co-occurrence'.

And who is the author of this passage? None other than Professor Roy Harris himself. (See his *The Language Machine*, p.69). We have here Harris saying one thing in a recent book, but then saying exactly the opposite in his reply to me - apparently just for the sake of scoring a cheap debating point. I accede here to editorial pleas for restraint, and simply invite readers to draw their own conclusions about scholarly standards.

As I said, having now taken a close look at Bloomfield, I see that he does find a place for the theory of meaning in linguistics. What he says is that semantics is the most recalcitrant area of linguistics, the mastery of which will require a great deal of scientific enquiry. So, perhaps inadvertently, Harris, in his reply to me, manages to say something correct. I wish to congratulate him on the nine lines of his paper in which he describes the place of semantics in Bloomfield's linguistic theory. This may be only a small matter in the context of this symposium, but truth is important, and Professor Harris' clarification is most welcome. Amidst the suffocating obfuscation and cancerous falsification of his paper it gleams like a little ray of sunshine.

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